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Hence the position angle p of the spot, its angular distance σ from the shadow axis and its linear distance r' from the Earth's centre are

$$\begin{aligned} r' \sin\sigma \sin p &= x - \xi \\ r' \sin\sigma \cos p &= y - \eta \\ r' \cos\sigma \qquad &= z - \zeta. \end{aligned}$$

If $\cos \epsilon_0$ is the proportion of the Earth's polar axis to the diameter of the equator

$$\sin \epsilon = \sin \epsilon_0 \cos \mathrm{D}$$

$$\tan \epsilon' = \tan \epsilon \cos p$$

$$h = \frac{\mathrm{K} - k}{\mathrm{R}},$$

the radius of the section of the geometrical umbra is

$$r' \sin \sigma = \cos \epsilon' - h \,.\, r' \cos \sigma.$$

But the observed enlargement of the shadow proves that while a lunar spot near the border of the shadow receives still, or receives already, direct sunlight from a cusp outside the Earth's atmosphere, this illumination is so feeble that, as seen from the Earth, it merges into the dim illumination produced by the broken sunlight passing through the atmosphere and surrounding part of the Earth's disc. The uncertainty in fixing upon what ought to be considered the true border-line of the full shadow leaves it doubtful how much of the light near the margin of the Sun is to be treated as practically ineffective, or how much the value of K must be diminished in order to furnish the border-line. If I am not prevented from preparing ephemerides for physical observations of the Moon for 1898, I intend to give, for a number of lunar spots, predictions of the approximate times of their crossing the border-line of the shadow during the three lunar eclipses of that year, in the hope that many observers may join in making contributions for the closer examination of the question.

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The Orbit of δ Cygni. By S. W. Burnham.

The companion to δ *Cygni* was discovered by Sir William Herschel on 1783 September 20. The angle was measured, but all we know of the distance of the components at that time is found in his description of the appearance of the star in his reflector. Herschel says, "With 278 about $\frac{1}{2}$-diameter of L [large star]; with 460 $\frac{3}{4}$-diameter of L; with 932 full $1\frac{1}{2}$-diameter of L in hazy weather, which has taken off the rays of

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L, and thereby increased the interval. Position, 18° 21′ *nf.*; perhaps a little inaccurate."

In the early part of the present century, Herschel examined this star several times, but could not see the companion. Referring to the matter at this time (*Phil. Trans.* 1804), he says :

"This double star, I believe, has furnished us with a second instance of a conjunction resembling that of ζ *Herculis.* The position, 1783 September 22, was 18° 21′ *nf.* 1802 January 3, 10 and 11, I could no longer perceive the small star, which must have been at least so near the large one as to be lost in its brightness. 1804 January 29, I examined this star with powers from 527 to 1500, and saw it as a lengthened star, but not with sufficient clearness to take a measure of its position. 1804 May 22, in a very clear evening, I tried 527 and 1500 with the 10-foot reflector, which acted remarkably well on the double stars, but I could not perceive the small star of δ *Cygni.* . . . A parallactic motion of δ will perfectly account for this occultation, for the situation of the two stars in 1783 was such that this motion must have carried the large star by this time nearly upon the small one."

It had been stated that Herschel was not able to see the companion at any time after his first observation, although he looked for it several times in the early years of this century; but Sadler has examined his MS. now in the Royal Astronomical Society (*Observatory*, September 1886), and given extracts from it relating to his observations in 1802–3–4, from which it appears that this star was seen double at least once on 1803 May 28, with the 7-foot reflector, and noted, "near ½-diameter of smaller. The diameters are full 2 to 1."

These and other failures to see the companion, taken in connection with the opinion of Herschel that the star had actually become single, led to many erroneous ideas concerning the supposed rapid motion. Struve thought that more than a whole revolution had been performed between 1783 and the commencement of his observations in 1826, and that the period was less than 40 years.

It soon became apparent that the motion was very slow and practically uniform, and that there had been but little, if any, change in the distance from the time of discovery. The failures to see the companion were then frequently charged to variability in its light, and this notion is still entertained by some observers. Judging from my own experience with this and similar unequal pairs in the last twenty years, and considering the value of negative evidence in matters of this kind, there is nothing whatever to warrant even a suspicion that there has been at any time any sensible variation in the magnitude of the companion. Dawes was sceptical as to the alleged variability, and said that "great allowance must be made for the effect on such an object of different conditions of the atmosphere"; and that he had

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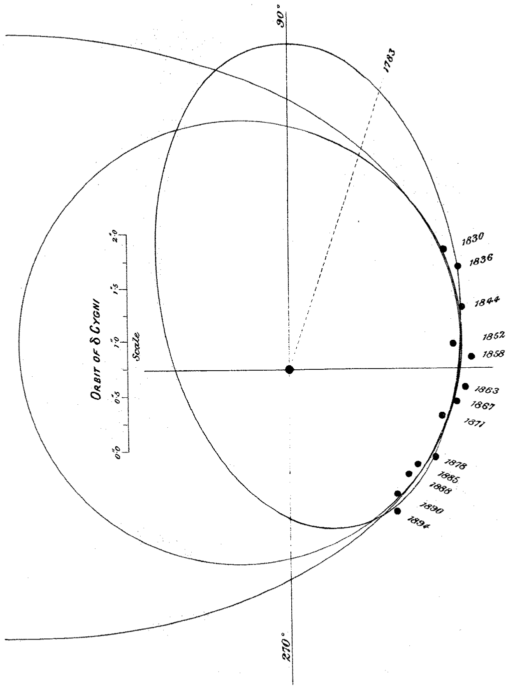

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"never viewed this star under a moderately favourable state of the air without seeing the small companion plainly enough with apertures from $6\frac{1}{3}$ to $8\frac{1}{4}$ inches." In a pair of this class, the widely differing estimates of the magnitude of the smaller star by different observers is no evidence of any change in its brightness. Such double stars are frequently not seen with small apertures which under proper conditions show the companion well.

Four orbits have been computed for this pair in the last fifty years. The dates of the last measures used are given in the first column :—

1842	Hind	178·7 years	*A. N.* 565
1856	Behrmann	280·56 „	*A. N.* 1517
1865	Behrmann	415·11 „	*A. N.* 1561
1887	Gore	376·66 „	*Mon. Not.* L, 398

How few data were available in 1842, when the arc described from Struve down to that time was only about 15°, and there was nothing else but a rough single measure of the angle which did not purport to have any great value, is too obvious to require comment here. If Herschel's angle had been supplemented with the distance, and in both the highest accuracy had been reached which is attainable by a mean result from measures on a large number of nights by the best modern appliances, there still would have been nothing in the material to limit the arc representing these positions to a single ellipse, or to several wholly dissimilar ellipses. The material in 1856 for all practical purposes was no better, although the position-angle had changed a few degrees in the interval of fourteen years. Behrmann gave an ephemeris to 1880, and it is not surprising to find that it turned out, when the real position of the companion was determined, that the calculated place for the last-named date was 170° in error. Nine years later, with a few degrees of further change in the angle, he made a new investigation and obtained the second orbit given above, finding an entirely different ellipse, and increasing the period 135 years. This represents Herschel's angle of 1783 without error, and makes the distance at that time 2″·56. Gore's orbit also satisfies this angle within 0°·7, and makes the distance 2″·72.

On the accompanying diagram (Plate 10) I have given the angle of Herschel and thirteen repeatedly measured positions by leading observers at short intervals down to the present time. Of course many other measures might be included, but they would serve no purpose so far as the present question is concerned, and add nothing to the weight of the evidence bearing upon the orbit of this system. In fact, putting in all the measures, good, bad, and indifferent, would only exaggerate the uncertainty about this star, which is already sufficiently apparent from a use of the best material only.

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I have shown on the diagram three possible orbits of this system, not with the idea or purpose of adding anything to our knowledge of the motion, but rather to emphasise the wholly indeterminate character of the problem presented. The arcs of the large ellipse and the circle are identical so far as the complete measures are concerned, and represent these positions as perfectly as they are ever likely to be represented, even when a complete revolution has been performed. The accuracy with which the measures are represented by these two entirely different orbits will appear from the following corrections to the observed positions.

	°	″		
1830·21	+0·1	−0·02	6*n*	Σ.
1836·52	+0·1	−0·09	4*n*	Σ.
1844·78	+0·8	−0·03	3*n*	OΣ.
1852·70	+1·3	+0·08	3*n*	OΣ.
1858·71	−1·3	−0·09	3*n*	OΣ.
1863·74	+0·7	−0·07	3*n*	OΣ.
1867·60	+0·2	−0·02	7*n*	Δ.
1871·69	−0·3	+0·05	9*n*	Δ.
1878·59	+2·5	−0·05	3*n*	Δ.
1885·73	−2·8	+0·06	4*n*	Hl.
1888·93	−3·4	+0·08	4*n*	Hl.
1890·57	+1·5	+0·03	4*n*	Hl.
1894·84	−0·8	−0·07	11*n*	Com.

These observations can also be represented precisely as well by an ellipse with the principal star outside altogether.

The smaller ellipse differs but little from the other figures, and for all practical purposes represents the measures equally well, since the largest distance variation does not exceed 0″·08. In this ellipse the distance in 1783 would be 2″·68, and the correction to the angle of Herschel is only 0°·5. And yet, aside from the short arc represented by the measures from 1830 to 1894, which is practically identical in all, these three orbits have nothing in common either as to the period or any other element.

The large ellipse gives a period of 1040 years, the circle 420 years, and the small ellipse 300 years. The true period may be anywhere between these limits, or perhaps outside of both. It must be remembered that in dealing with a short arc the agreement of observed and computed positions, however close and satisfactory it may be, does not even tend to prove that the orbit so found is correct, or anything like the real orbit. It is doubtful if the measures of another hundred years will be sufficient to determine the period of this pair to a reasonable approximation.